

*A Dissertation concerning the Figure of the Earth,  
by the Reverend John Theophilus Desaguliers,  
LL. D. F. R. S.*

THAT the Earth (that is, our Terraqueous Habitation) is of a spherical Figure, or nearly such, has been prov'd so often, and by so many unanswerable Arguments, that to repeat them here must needs be tedious to this learned Society. But, as a little Variation from a true Sphere (besides the Irregularity of high Hills and deep Vallies) does not hinder us from calling the Earth a Globe; so, to determine what that Variation may be, since modern Philosophers are divided about it, may be a Subject not ungrateful at this Time.

Monfieur *Cassini* says: “ That the Earth is an oblong Spheroid, higher at the Poles than the Æquator, making the Axis longer than a Diameter of the Æquator about thirteen *French* Leagues, which he deduces from comparing his Father’s Measures of the Meridian, from *Paris* to the *Pyrenæan* Mountains, with those of Monfieur *Picard*; of which an Account may be seen in the Memoirs of the Royal Academy for 1713. But having afterwards continued the Meridian, which is drawn through *France*, from *Paris* to *Dunkerque*, he still draws Consequences to prove the Earth an oblong Spheroid; but then makes the Axis exceed the Æquatorial Diameter 34 Leagues.

“ Sir *Isaac Newton* makes the Earth higher at the  
 “ Æquator, and, consequently, flatted towards the  
 “ Poles, reckoning its Æquatorial Diameter 34 *English*  
 “ Miles longer than the Axis; which he proves from  
 “ the Principles of Gravity, and the Centrifugal Force  
 “ that arises from the Diurnal Rotation of the Earth;  
 “ and, to confirm this, mentions several Experiments  
 “ on Pendulums, which have been made shorter, to  
 “ swing Seconds, near the Æquator, than in greater  
 “ Latitudes.”

These are the two Opinions which have divided Philosophers, and which we propose to examine here.

Monfieur *Cassini*, taking the Measures above-mentioned to be exact enough, not only to determine the Magnitude of a Degree of the Earth, corresponding with a Degree of the great Circle of the Heavens, but also to shew the Difference in the Degrees of the Earth; (reckoning those, that were measured in the South of *France*, to exceed those towards the North, by a certain Number of Toises and Feet) demonstrates, that if the Degrees of the Earth are longer towards the Æquator than the Poles, the Plane of the Meridian must be an Ellipse, whose long Axis is that of the Earth. Here follows his first Demonstration. [*See the French Memoirs for the Year 1713*]

“ Let B D C R \* be an Ellipse that represents a  
 “ Meridian of the Earth, whose Poles B and C are at  
 “ Ends of the great Axis B C, and whose Foci E and  
 “ F are taken at Pleasure. Now, to divide this  
 “ Ellipse into Degrees, that is, to find several  
 “ Points H, I, V, such, that the Distance, from the  
 “ Pole to the Zenith, of every one of them, shall  
 “ be of any given Number of Degrees.

\* Fig. 1.

“ From E, one of the Foci of the Ellipse, draw the  
 “ Line E T, so that it may, with the Axis B C,  
 “ make the Angle B E T equal to the Distance given  
 “ from the Pole to the Zenith. From the other Focus  
 “ F, with the Distance B C equal to the Axis, draw  
 “ an Arc, to cut the Line E T at T. I say, that the  
 “ Line F T, drawn from the Point T to the Focus  
 “ F, will cut the Ellipse at the Point H; which Point  
 “ is such, that the Distance of the Pole, from its  
 “ Zenith, contains the given Number of Degrees.

### “ D E M O N S T R A T I O N .

“ From the Point H, raise H Z, perpendicular to  
 “ the Ellipse which will pass through the Zenith Z;  
 “ and, being produc'd inwards, will meet the Axis of  
 “ the Earth at O, and (by the Property of the Ellipse)  
 “ divide the Angle E H F into two equal Parts. From  
 “ the Point H, draw likewise H P, parallel to the Axis  
 “ B C, and directed to the Pole P, suppos'd at an infi-  
 “ nite Distance. The Angle P H Z, or P O Z, mea-  
 “ sures the Distance, from the Pole to the Zenith, of  
 “ an Inhabitant dwelling upon the Earth at the Point  
 “ H. F T is equal to the Axis B C, by Construction;  
 “ but, by the Property of the Ellipse, B C is equal to  
 “ E H *plus* H F; taking away from both F H, which  
 “ is common, E H will remain equal to H T. The  
 “ Angles E T H, T E H, will therefore be equal,  
 “ and, consequently, each of them will be half of the  
 “ external Angle E H F; but the Angle E H O is  
 “ likewise equal to half of the Angle E H F; there-  
 “ fore the Angles T E H, E H O, will be equal to  
 “ one another; and, consequently, the Lines E T and  
 “ H O will be parallel to one another; and the Angle  
 “ P O Z, which measures the Distance from the Pole

“ to the Zenith of the Point H, will be equal to the  
 “ Angle B E T, which was, by Construction, taken  
 “ equal to the given Distance of the Pole from the  
 “ Zenith ; *which was to be demonstrated.*

“ Now, if the Proportion of the longest Diameter  
 “ of the Ellipse B C to E F, the Distance of the Foci,  
 “ be taken at Pleasure, one may by Calculation  
 “ find all the Points of the Ellipse as H, to deter-  
 “ mine the Degrees by this Analogy.

“ *As F T, or B C:*

“ *Is to E F ::*

“ *So is the Sine of the Angle P E T (the given Di-*  
 “ *stance from the Pole to the Zenith) :*

“ *To the Sine of the Angle E T F, or T E H :*  
 “ whose Quantity will consequently be known. This  
 “ Angle T E H being added to the Angle P E T, the  
 “ given Distance from the Pole to the Zenith of the  
 “ Point H, will give the Quantity of the Angle B E H,  
 “ which a Line drawn from the Focus to H, the Point  
 “ requir'd, makes with the Axis of the Ellipse.

“ Then in the Triangle E H F, whose Side E F is  
 “ known, as well as the Angle E H F, which is the  
 “ Double of the Angle T E H, and the Angle F E H  
 “ Supplement of the Angle B E H ; one shall have the  
 “ Length of the Side E H, known in Parts of the Axis  
 “ B C.

“ After the same Manner, may be found the Angles  
 “ B E I, B E V, &c. and the Length of the Lines E I,  
 “ E V, to determine the Distance, from the Pole to the  
 “ Zenith, of all the Degrees of the Circumference of  
 “ the Earth ; and in the rectilinear Triangles H E I,  
 “ I E V, whose Sides H E, E I, E V, are known as  
 “ well as the Angles comprehended between the Sides  
 “ H E, E I, I E, E V, which are the Differences of  
 “ the Angles B E H, B E I, B E V, determin'd above ;

“ one

“ one shall find the Length of the Chords H I, I V,  
 “ comprehended between each Degree.

Monf. *Cassini*, in the Memoirs for the Year 1718, repeats the same Demonstration ; except that, before it, he shews, that if several Points be taken upon a Terrestrial Meridian, on the Surface of an Elliptick Earth, as G, H, I, K,\* in such Manner, that their respective Zeniths Z, L, M, N, are distant from one another, an equal Number of Degrees measur'd in a Celestial Meridian. The Lines Z G, L H, M I, N K (which are perpendicular to the Ellipse) being produced, will meet in the Points O, R, and S, making equal Angles; but as those angular Points are not equally distant from the Curve of the Ellipse, that Elliptic Arc must be the longest whose angular Point is farthest off. Now, by the former Demonstration, it appears, that those Arcs, which are taken nearest to the lesser Axis, will have their angular Points farther remov'd, &c.

If Monf. *Cassini*'s Measures of Terrestrial Degrees, decreasing from the *Æquator* towards the Pole, were grounded on Observations liable to no Error, he wou'd have fully prov'd his Figure of the Earth. But since those Measures (however accurately taken) are not built upon a mathematical Certainty, his Premises may be call'd in Question; and his Conclusion, tho' mathematically drawn from these Premises, is only probable.

Now therefore, if I can shew from undoubted *Phænomena*, that his Conclusion will lead to an Absurdity, his Measures must be false ; because his Reasoning from them is just. This I shall endeavour to do first, which will disprove his Figure of the Earth ; and afterwards endeavour to point out some of the Errors which

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I suppose to have occasion'd the Mistake in the Measures.

Monf. *Cassini*, as well as the *English* Astronomers, believes that the Earth makes one Revolution about its Axis, once in 23 Hours 56', because in that Time, the Plane of any Meridian returns to the same fix'd Star from which it had departed.

Let \* H be taken in any Parallel of Latitude, as for Example, in the Latitude of  $51^{\circ} 46'$ , a Plumb Line, L H, will be perpendicular to the Curve B H, at H, and produced pass thro' the Zenith of the Point H, if the Earth had no Diurnal Rotation; but since the Earth moves round its Axis, all Bodies upon its Surface, endeavour to fly from the Axis of their Motion with a Force proportionable to their Distance from it in a Direction along the Plane of that Parallel, in which they are. Let that Force (explain'd by Monf. *Huygens*, and called a Centrifugal Force) be represented by the Line H I, or its Equal and Parallel L b; now a Plumbet placed at L, if the Earth stood still, would descend in the Line L H, but as it is at the same Time acted upon by the Force H I in the Direction L b, it will move in the Direction L I Diagonal of the Parallelogram H I, according to the known Laws of Mechanicks; and the Plumb Line L H, instead of being perpendicular to the Curve at H, will in the Latitude  $51^{\circ} 46'$  make an Angle of 5' with H L. This Angle will be less towards the Poles, till at the very Pole it quite vanishes, as it also does at the Equator. *The Demonstration of the Quantity of this Angle, I shall give before I end this Dissertation.* Now since there is no such Angle observ'd, but in all Water Levels we find the Plumb Line always perpendicular to the Line of Level, the Surface

Surface of the Earth must be depress'd towards G, and rise farther from the Axis towards I, in order to become perpendicular (that is, to have its Tangent perpendicular) to the Line *L I*, in which we have shewn that the Plumb Line must descend.

If there is any Body so fond of Monsieur *Cassini's* Hypothesis, as to deny the Diurnal Motion of the Earth for the sake of it, I hope they will be convinc'd, when I shew the Measures, upon which it is founded, to be insufficient for determining the different Lengths of the Degrees of a Terrestrial Meridian.

But here I would not be thought to endeavour to lessen the Praise due to the Gentlemen of the *Royal Academy*, for carrying on a Meridian the whole Length of *France*, from *Dunkerque* thro' the Royal Observatory at *Paris*, quite to the *Pyrenean* Mountains on the Borders of *Spain*. Astronomy and Geography are doubtless much indebted to the Encouragement given by the *French* Government, and to the Care of their Mathematicians, who have omitted no proper Method for drawing their Meridian, and correcting it as they went on. So many Observations of the rising and setting Sun, so many equal Altitudes of the same Stars accurately taken, so many Digressions of Stars, so many other Observations made with the Telescope and good Pendulum-Clocks — all compar'd together, for the true settling of the direct Way of this famous Meridian, leave no Doubt but that it is as perfect as the Nature of the Thing is capable of. And, certainly, by the Help of this Line, and the several Triangles made use of for carrying it on, a better Map of *France* is made, than has ever been of any Country before: Nay, besides, I believe we may, at a Medium, very well receive their Number of 57060, or 57061 Toises, for the Measure of a Degree of a Meridian of the Earth,

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one with another. But to say, that those Gentlemen cou'd observe the Latitude so nicely, as to find a Difference in the Length of the Terrestrial Degrees, and that only of eleven or twelve Toises, (when they made it the least) or of thirty-one Toises, (when they made it the most) is attributing to them an Exactness, so far beyond the Nature of the Instruments which they made use of, that it wou'd be rather a Dispraise than a Commendation to insist upon it.

For in the first Place, the Instrument, with which they took Observations for the Latitude at the two Ends of their Meridian, was a ten Foot Sector (which was worse than that which Monf. *Picard* had made Use of before, because the Telescope of his Sector was of ten Foot, whereas Monf. *Cassini's* was but of three Foot, tho' applied to the ten Foot Sector) where the two hundredth Part of an Inch answers to eight Seconds of a Degree : Now the two hundredth Part of an Inch, being one of the least visible Parts that we can see in a divided Line ; they could not take an Angle nearer than that ; nay, their Instrument, according to their own Description of it, was divided but to every twenty Seconds. Now they allow, that sixteen Toises, upon the Surface of the Earth, answer to one Second in the Heavens ; and they don't pretend to have taken an Observation nearer than to about three Seconds, which therefore cannot determine a Difference less than forty eight Toises ; whereas the Degrees are only supposed to decrease at most, thirty one Toises each, from *Collioure* to *Dunkerque*. But an Error of eight Seconds wou'd make a Difference of one hundred and twenty eight Toises, on the Surface of the Earth ; above ten Times greater than the Difference of Degrees in the first Supposition, and four Times greater than that Difference in the last. Besides, the Latitude was not observ'd in the intermediate Places  
be-



between *Paris*, and *Collioure*, with the abovemention'd Instrument of ten Foot Radius; but they made Use of a Quadrant, whose Radius was only thirty nine Inches, and sometimes an Octant of three Foot Radius. Nay, they say themselves, in their Account, that it is not the Observations made at the Ends of the Meridian, that we are to deduce the Difference of the Length of a Degree from, but the Altitudes taken at several Places between the Extreame; and, if we grant, that they can take an Angle very well, to four or five Seconds, with the great Instrument, they cannot come nearer than twelve or fifteen Seconds, with the Quadrant or Octant, which we must depend upon for the Difference of the Measure of Degrees: So that upon the whole, we are to determine a Length of thirty one Toises, by an Instrument which is liable to err above two hundred.

If any Consequences of this Kind cou'd be drawn from actual measuring, a Degree of Latitude shou'd be measur'd at the *Æquator*, and a Degree of Longitude likewise measur'd there; and a Degree very northerly, as for Example, a whole Degree might be actually measur'd upon the *Baltick* Sea, when frozen, in the Latitude of sixty Degrees. — There, according to *Monf. Cassini's* last Supposition, a Degree wou'd be of 56653 Toises, whereas, at the *Æquator*, it wou'd be of 58019 Toises, the Difference being 1364 Toises, about the two and fortieth Part of a Degree, which must be sensible; and likewise the Degree of Longitude wou'd, according to him, be of 56817 Toises, less by 1202, or the forty eighth Part, than a Degree of Latitude at the same Place.

But here it may be objected, that tho' the Latitude was not taken with the ten Foot Sector, in the intermediate Places between *Paris* and *Collioure*, yet the

Latitude was taken with that Instrument at *Dunkerque*, *Paris*, and *Collioure*, and therefore the southern Part of the Meridian, containing  $6^{\circ} 18' 56''$  may be compar'd with the northern Part of it, which contains  $2^{\circ} 12' 16''$ ; and that the former appears to contain more Toises, in Proportion to the Difference of Latitude at its Extremities, than the latter. To this may be answer'd, that, even in this Case, the Observations made cannot be nice enough to determine the Difference of the Length of Degrees; but there is another Error, which might considerably mislead the *French* Gentlemen, and make the Degrees appear longer in the South of *France*; that is, the Error in taking the true Height of several Mountains in *Auvergne*, *Languedoc*, and among the *Pyreneans*. For if they have allow'd too much for the Air's Refraction (which, by the Observations of Travellers, is greater towards the northern Regions, and diminishes as we go Southward) the Heights of those Mountains will be taken too little, and their Bases consequently longer, which will make the Degrees appear bigger than they are. Let A B C D, \* for Example, be a Mountain, as the Mountain of *Rodex*, in the Latitude of  $44^{\circ} 21'$ , whose Height B D is 300 Toises, and whose Sides A B and B C (suppos'd to make an Angle of  $26^{\circ} 33'$ , with the Horizon) are found by Trigonometry, to be of 670 8 Toises each; if by a Mistake, in taking the Height, it be suppos'd only equal to E D, or 257 Toises, then the Lines A B and B C will become E F and E G; so that the Base A C, which before was of 1200 Toises, will become equal to F G, which will appear to be 1279,6 Toises, by *Euc.* 47. 1. Now one such Mistake, in one Degree, will give a Difference above twice as great as the suppos'd Difference

rence of Degrees in that Latitude, which they make of 31 Toises. And that there was a Mistake of this Kind in taking the Height of that Mountain, I shall shew.

The Vapours, that generally float in the Air about the Tops of high Hills, make it so difficult to take their Height exactly, that Experiments, made with the Barometer, will, by observing the Fall of the *Mercury*, shew the Height nearer than any Thing else we know of. There were, indeed, several Experiments made with the Barometer,\* where the Differences of the Height of the *Mercury*, from the Heights at which it stood at the *Royal Observatory*, are said to answer to so many Toises ; but of nine Observations mention'd by Mons. *Cassini*, there are not two where the Number of Toises, said to correspond to the Heights of the Barometer, do agree together.

The first Experiment of the Barometer there mention'd, made at *Collicure*, was this, “ At the Height  
“ of  $11\frac{1}{2}$  Toises above the Sea, the Barometer was set  
“ up, and the *Mercury* stood  $3\frac{1}{2}$  Lines higher than at  
“ the *Royal Observatory* (in the Tower of the eastern  
“ Hall) at the same Time ; and therefore, since that  
“ Tower is 44 Toises higher than the Sea,  $3\frac{1}{2}$  Lines  
“ of *Mercury* must answer to  $32\frac{1}{2}$  Toises.

Now, reducing these Toises to Feet, and dividing by  $3\frac{1}{2}$  it will appear that an Height of 58,5 Feet will answer to the Fall of one Line of *Mercury* in the Barometer. Let this be taken as the Standard, and the other Observations be compar'd with it. This may be done by the following Table, where the first Column shews the Place where the Observation was made ; the second, the Fall or Rise of *Mercury* at each Place express'd in Lines, or 12th Parts of a *French* Inch ; the

H. h 2	third,
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\* *Mem. of the Royal Academy, for 1718, ch. 10.*

third, the Heights or Depths answering to those Lines of *Mercury*, which, in the Memoirs, are given in Toises, but here reduced to Feet ; the fourth, the Number of Feet answering to one Line of *Mercury* in each Observation, which is the Quotient of the Feet in the third Column, divided by the Number of Lines in the Second:

<i>Observations of the Barometer made at</i>	<i>Lines of Mercury.</i>	<i>Said to correspond with Feet.</i>	<i>The Fall of one Line of Mercury answers to Feet.</i>
I. Collioure - -	03 $\frac{1}{2}$	195	58,5
II. The Tower of Massane.	31	2382	76,8
III. Bugarac -	42	3636	86,5
IV. Rupeyroux. -	30	2181	72,7
V. Rodez -	24	1647	68,6
VI. Rodez -	20	1425	71,25
VII. Courlande -	54	4812	89,1
VIII. Coste -	54	4890	92,4
IX. Clermont -	03	200	66,6

A Sight of this Table will convince any one, that these Observations are not to be depended upon for determining the Height of the Mountains in the South of *France* ; for the Differences are not small, such as might happen in making the Experiments ; but such as render the Observations usefess for the Purposes above-mention'd. For Example, the first and the seventh differ almost  $\frac{1}{2}$  : And if 58,5 Feet were allow'd for the Fall of one Line of *Mercury* in the seventh Observation, instead of 944 Feet, then the Mountain of *Coste* would be but 3085 Feet, instead of 4890. Nay, upon examining the Memoirs, I find that in several Observations the Number of Toises, said to correspond to a certain Height of *Mercury*, are only answerable to the Height of the Mountain above the Level of the Sea found by Trigonometry, from which the Height of the *Royal Observatory*, above the Sea, is subtracted ; though, by the Manner of the Expression, a cursory Reader would imagine

imagine, that the Number of Toises nam'd, was always proportionable to the Fall of the *Mercury*, and think all the Experiments and Observations very accurately made, when they seem to agree so well in every Respect.

Now after all, I do not question but that the Height of the Barometer, might be as it is set down in the Memoirs, and well enough observ'd; but it was wrong to compare the Height of the *Mercury* in the South of *France*, with the Height that the *Mercury* was at in the Barometer of the *Royal Observatory* at the same Time; for, at that great Distance and Difference of Latitude, the Weather (and consequently the Pressure of the Air and Height of the Barometer at the same Level) might very much vary.

Even when there is fair Weather all over *France*; it does not follow that the Barometer shall stand at the same Height: Let us suppose, for Example, that a North Wind blows: Where-ever the Air is check'd by a Chain of Mountains that run East and West, it will be accumulated over those Mountains, and consequently press more as its Columns are higher; which will make the *Mercury* rise higher than it wou'd do with the same Wind, if there were no Mountains, or if they ran North and South.

The Way, to have made the Experiments with the Barometer exactly, wou'd have been to have observ'd the Height of the *Mercury* at the Bottom and at the Top of the Mountain, and that with a Tube of a pretty large Bore (with a proportionably large Cistern for the stagnant *Mercury*) because, in a small Tube, the *Mercury* will often stick to the Sides, and rise irregularly, as it will also in inclin'd Barometers. Simple Barometers are the best, and a magnifying Glass may be made use  
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of

of to observe small Rises or Falls, having two fine and well made Indices to the Tube.

Dr. *Edmund Halley*, our Royal Astronomer, has, some Years ago, given us, in the *Philosophical Transactions*, the Falls of *Mercury* in the Barometer, corresponding with the Heights to which the Barometer must be carried to produce those Falls. The first tenth Part of an Inch in the Fall of the *Mercury*, he makes to answer to an Height of 90 Feet; the next tenth, to an Height something greater, and so in Proportion, as the Air diminishes in Density, according as we rise in Height. The Proportion of the first Tenth of the *Mercury's* Fall, he has built upon the Comparison of the different Specifick Gravities of Air and *Mercury*; and taking *Mercury* to be  $13\frac{1}{2}$  Times heavier than Water, and Water (in cold Weather) to be 800 Times heavier than Air; it follows, that  $13,5 \times 800$ , will give 10800; which Number, if it be taken in Feet, and divided by 120 (the Number of the 10th of an Inch in a Foot) we shall have 90 Feet answerable to the 10th of an Inch, and 75 Feet to a Line or the 12th Part of an Inch. As the Doctor's Tables may be of use to the Curious, I have inserted them here.

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I. *A TABLE shewing the Altitude, to given Heights of the Mercury.*

Inches.		0	Feet.
29		915	
28		1862	
27		2844	
26		3863	
25		4922	
20		10947	
15		18715	
10		29662	
5		48378	
I		91831	
0 . 5		110547	
0 . 25		129262	
0 . I	29 Miles or	154000	
0 . 01	41 Miles or	216169	
0 . 001	53 Miles or	278338	

II. *A T A B L E* shewing the Heights of the Mercury, at given Altitudes.

Feet.	0	30	00	Inches and Tenths.
	1000	28	91	
	2000	27	86	
	3000	26	85	
	4000	25	87	
	5000	24	93	
	1 Mile	24	67	
	2	20	29	
	3	16	68	
	4	13	72	
	5	11	28	
	10	4	24	
	15	1	60	
	20	0	95	
	25	0	23	
	30	0	08	
	40	0	012	

Now, as very few Mountains in the World are 3 Miles high, and, generally speaking, those that we look upon as high Hills, (except the *Andes*, and some others in *America*) are not much above a Mile high; we may, for finding the Height of Mountains, take a fix'd Number of Feet in Altitude to answer to every 10th or 12th of an Inch in the Fall of the *Mercury*; because 90 Feet are by Dr. *Halley* only taken for the first Tenth, and greater Heights for other Tenths, encreasing with the Fall of the *Mercury*. Therefore I wou'd propose another for a round Number, namely 96 Feet for every Tenth, and 80 Feet for every 12th of an Inch, very near the Number that I have found by my Calculation, which is as follows. Fine



Fine *Mercury* (such as is made use of in Barometers) is, generally speaking,  $13\frac{1}{3}$  times heavier than Water; and, I found some brought from the *East-Indies*, to be 14 times heavier. I have found Air in Summer, to be near 900 times lighter than Water; and 800 times in Winter; therefore I take 850 at a Medium. Now  $850 \times 13\frac{1}{3} = 11606,6$ , which, divided by 120, gives 96,7 Feet, for  $\frac{1}{12}$  of an Inch of *Mercury*, or 80,5 Feet for  $\frac{1}{16}$  of an Inch. This Number, taken invariable, will, in taking the Height of several Hills, agree pretty well with the Numbers that come out, when Dr. *Halley's* Table is made use of; and with the Experiment made by the late Professor, Mr. *J. Caswell*, who, having taken the Height of *Snowdon Hill* in *Caernarvan-shire* very accurately, and finding it to be 3720 Feet above the Level of the Sea, tried how much lower the *Mercury* wou'd stand in the Barometer upon that Hill, than at the Level of the Sea, and observ'd it to subside 39 Inches. I am sensible that it will be alledged, that the Air will be denser than I may imagine on the Top of high Hills, because of the great Cold, since they are generally cover'd with Snow; but then we are to consider, that when we are got above a Mile higher than the Level of the Sea, the incumbent Atmosphere has lost almost a 5th Part of its Weight\*; and therefore the Air at the Top of the Hill, being so much less press'd, will, notwithstanding the intense Cold, be more rarified than at the Bottom of the Hill.

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\* If the whole Air was reduced to the same Density, as it is near the Level of the Sea, the Atmosphere wou'd be but between five and six Miles high; whereas, in its present State of gradual Rarefaction, it is above 50 Miles high, as we find by several Phænomena of Meteors observ'd to float in the Air, so high at least.

Now if we go back to the Observations of the Barometer, made by the Gentlemen that drew the Meridian in *France*, we shall find, that on the Mountain of *Rhodesz*, in the Latitude of  $44^{\circ}, 21'$  the Barometer fell 24 Lines below the Level of that in the Observatory, and they allow'd only  $274\frac{1}{2}$  Toises to correspond to that Fall; whereas, according to Dr. *Halley's* Proportion of a Tenth of an Inch for 90 Feet, they shou'd have taken 300 Toises; and tho' the *Hypotenuses* AB, and BC, \* were taken longer than the bare Declivity of the Mountain (which wou'd make the Error less than the 79 Toises I mention'd above) yet if my Proportion be made use of, viz. of 80 Feet for each Line of *Mercury*, that will make the Mountain 320 Toises, which, being higher, will therefore shew the Base to be yet shorter, and consequently the Error, at that Rate, will be greater.

This Error (and such like, if any more were made) will encrease the Measure of the 44th Degree of Latitude on the Earth; and, by observing what was done in the next Degree, we shall find that that Degree was taken too short. In the Latitude of  $45^{\circ}, 38'$ , the Mountain of *Cosse* is made 815 Toises high; whereas the 54 Lines of the falling *Mercury* in the Barometer, said to answer to that Height, will give but 705,6 Toises (which we will call 705,5) even according to my Computation of 80 Feet to a Line, which is the greatest Allowance. If we suppose this Mountain to rise in an Angle of  $26^{\circ}, 33'$ , as we did that of *Rhodesz*, the Sides of the Mountain, or *Hypotenuses* AB, and BC, † will be each equal to 1577,54 Toises, and the whole Base AC, to 2822 Toises. Now, when the Height of this Mountain is call'd 815 Toises, the Base AD, or DC

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\* Fig. III.

† Fig. IV.

(*by Eucl. 47. 1.*) becomes only equal to  $FD$  or  $DG = 1350,7$  Toises; and its Double, or  $FG$  the whole Base, will be but  $2701,4$  Toises, less than the former by  $120,6$  Toises. This Error is so great (so much more than the Difference of  $31$  Toises for a Degree) that tho' I suppos'd the Lines found by Trigonometry, which terminate at the Top of the Mountain, to be much longer than the Hypotenuse  $AB$ , yet there will be Error enough to make the  $45^{\text{th}}$  Degree of Latitude appear much shorter than it is. Supposing (because of the Length of the Lines  $AB$ , or the great Distance from which the Mountain might be observ'd) that these Errors were four times less than I made them; yet, at that Rate, one must add near  $20$  Toises to the  $44^{\text{th}}$  Degree of Latitude, and take away above  $30$  from the  $45^{\text{th}}$  Degree, which will make the  $44^{\text{th}}$  of  $57080$  Toises, and the  $45^{\text{th}}$ , of only  $57030$ ; and this will give a Difference of  $50$  Toises; so that if an Angle can be taken to two or three Seconds, to which  $32$ , or  $48$  Toises, are said to answer upon the Surface of the Earth, such a Difference might be visible.— And much more so, if other Errors of the same Kind shou'd happen to have been made the same Way; or if those Errors were nearer my first Supposition than this last. Nay, tho' the  $45^{\text{th}}$  Degree of Latitude may be  $13$  Toises bigger than the  $44^{\text{th}}$ , it might by this Means appear to be considerably less.

Such a Mistake might be the Occasion of making the Hypothesis of the Earth being an oblong Spheroid, especially because in this Hypothesis, the Degrees differ most in Length from one another about the  $45^{\text{th}}$  Degree; and, when once an Hypothesis is set on Foot, we are too apt to draw in Circumstances to confirm it; tho', perhaps, when examin'd impartially, they may rather weaken, than strengthen our Hypothesis; otherwise, the Author of the History of the *Royal Academy*, for the

1713, wou'd not have alledg'd, that *the late Mons. Cassini observ'd Jupiter to be oval*, as a Proof of young Mons. *Cassini's* Hypothesis ; because *Jupiter* is oval the other Way, that is, an oblate Spheroid flatt'd at the Poles, as the said late Mons. *Cassini* gave the Proportion of the Axis, to the Æquatorial Diameter, to be as 15 to 16.— And our late ingenious Astronomer, the Reverend Mr. *Pound*, with a Telescope of 123 Foot *Focus*, and an excellent Micrometer, has given those Proportions as 11 to 12. If a Proof is to be drawn from Analogy, or what is observ'd in other Planets, this must destroy Mons. *Cassini's* Hypothesis, and confirm Sir *Isaac Newton's*.

The Opinion of Dr. *Burnet* (the Theorist) quoted in the Memoirs, for the Year 1713, is but a very weak Argument in favour of Mons. *Cassini's* Hypothesis, on Account of the Reason Dr. *Burnet* gave, to prove the Earth higher at the Poles, than the Æquator ; for he says, “ That the Velocity of the Parts of the Earth, “ in its Diurnal Rotation, being greater at the Æqua- “ tor than towards the Poles, all the Water must be “ driven towards the Æquatorial Regions ; from “ whence, being repelled by the Resistance of the “ Air, it must run off again towards the Poles ; and “ so the Figure of the Water was lengthen'd out into an “ oblong Spheroid, and consequently the Crust of the “ Earth over it did put on the same Figure, &c.

But why the Air shou'd resist more towards the Æquator than the Poles, the Doctor did not give any Reason to shew ; and, if it had been so, the same Force, that drove the Water towards the Æquator, must have kept it there. The Doctor, in the latter Part of his Assertion, forgot what he had said in the former ; for the Water cou'd not run off towards the Poles, whilst the Earth continued its Rotation with the same Velocity.

For

For if he had consider'd, he wou'd have found his Argument in other Words to be this. *Because Bodies, that move in a Circle, always endeavour to recede from the Axis of their Motion; therefore the Water, by that Endeavour, comes nearer to the Axis of its Motion*; which is absurd. But Dr. Burnet, afterwards, alter'd his Opinion, as I am credibly inform'd.

Having thus given my Reasons for disapproving of Mons. *Cassini's* Opinion, concerning the Figure of the Earth; I come now to consider Sir *Isaac Newton's*, who makes it higher at the Aequator, than at the Poles; but before I enter upon it, I beg Leave to quote a Paragraph out of the History of the *Royal Academy* for 1713. These are the Words of the Author. "Reasonings drawn from the different Lengths of a Pendulum in different Climates, or from the Inequality of the Centrifugal Force arising from the Diurnal Motion of the Earth, are, perhaps, too nice to produce a certain Conviction; nay, perhaps, we are not well enough assured of the Principles, and the Consequences may sometimes be different. And therefore it is evident, that the best Way in this Enquiry, is only (as Mons. *Cassini* does) to make use of unquestion'd Observations, which serve directly to decide the Question.

That Mons. *Cassini* has not made use of *unquestion'd Observations*, and the Measures, he mentions, are not able to decide the Question, appears from what I have already said. We must therefore shew, whether the Principles, from which Sir *Isaac Newton* has deduced his Figure of the Earth, are fully prov'd or not: Whether the Conclusion drawn from them is plain and evident; and whether the Experiments on Pendulums, that confirm the Theory, are easy to be made, and may be depended upon.

Tho'

Tho' Sir *Isaac Newton*, in his *Principia*, has not endeavour'd to give the Cause of Gravity, or to determine whether it be owing to an Impulse or not ; yet he has shewn what its Effects and Laws are, from plain Experiments made by others and himself. From the Laws of Gravity, and from the Observation of a Comet, \* he has deduced the Annual Motion of the Earth ; and it must have a Diurnal Motion, if it has an Annual one, otherwise, it will not agree with the *Phænomena*. The Laws of the Centrifugal Force, or that Force by which a Body, whirl'd round in any Circle, endeavours to recede from the Center of its Motion, have been demonstrated by Monf. *Huygens*.

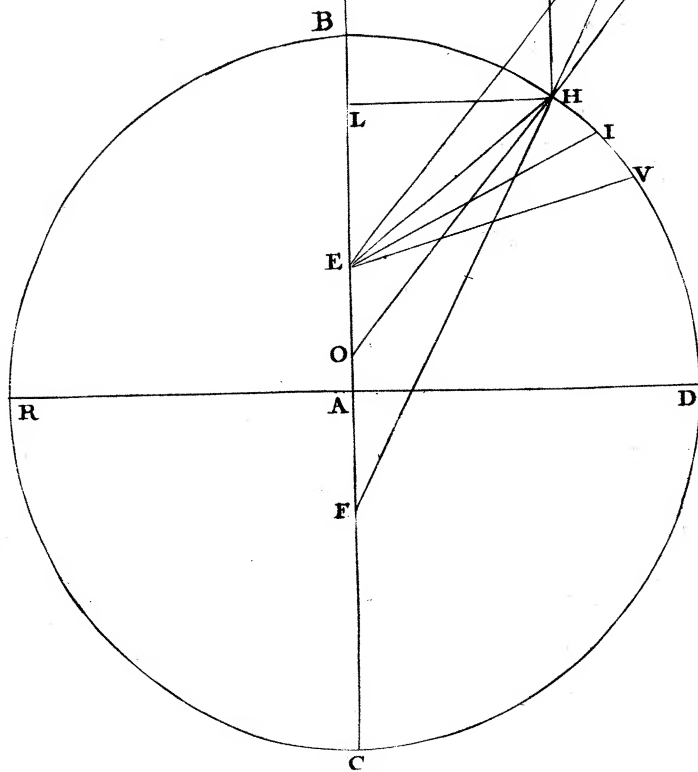
These are the Principles from which Sir *Isaac Newton* draws his Conclusion ; and tho' some Persons, that will not be at the Pains to examine them, may deny them by the Lump, yet no Body has yet been able to shew any Flaw in the Demonstrations that relate to them.

NB. *This Dissertation will be continued in our next.*

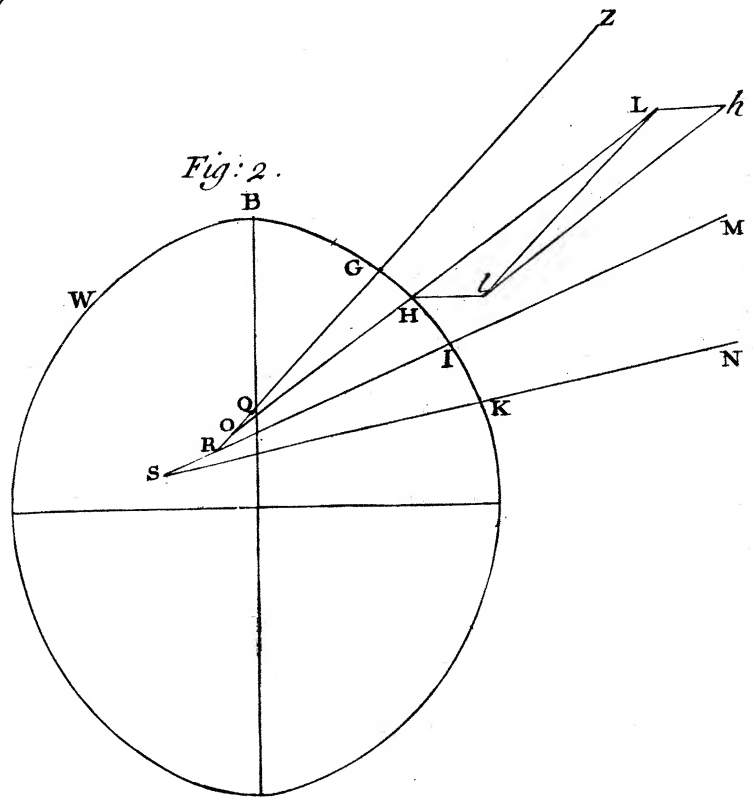
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\* *Princip. Lib. 3. Prop. 12, 13, & 42.*

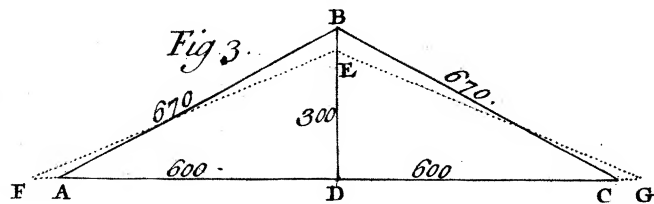
*Fig. 1.*



*Fig. 2.*



*Fig. 3.*



*Fig. 4.*

